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CONTENTS:

The Biological Problems of To-day:—

<i>Paleontology:</i> PROFESSOR HENRY F. OSBORN.	
<i>Botany:</i> PROFESSOR WILLIAM TRELEASE.	
<i>Anatomy:</i> PROFESSOR BURT G. WILDER.	
<i>Psychology:</i> PROFESSOR J. McKEEN CATTELL.	
<i>Physiology:</i> PROFESSOR JACQUES LOEB.	
<i>Developmental Mechanics:</i> PROFESSOR T. H. MORGAN.	
<i>Morphogenesis:</i> PROFESSOR CHARLES B. DAVENPORT.....	145
<i>Current Problems in Plant Morphology:—</i>	
<i>Relationship between Pteridophytes and Gymnosperms:</i> PROFESSOR CONWAY MACMILLAN.....	161
<i>Paleontological Notes:</i> H. F. O.....	164
<i>Current Notes on Anthropology:—</i>	
<i>Deformed Skulls from Guatemala; Native American Stringed Instruments:</i> PROFESSOR D. G. BRINTON.....	165
<i>Notes on Inorganic Chemistry:</i> J. L. H.....	166
<i>Scientific Notes and News:—</i>	
<i>International Congress of Zoology; General.....</i>	167
<i>University and Educational News.....</i>	172
<i>Discussion and Correspondence:—</i>	
<i>'Wild Neighbors:'</i> ERNEST INGERSOLL, VERNON BAILEY.....	172
<i>Scientific Literature:—</i>	
<i>La Cellule et les protozoaires:</i> GARY N. CALKINS.	
<i>Sleep:</i> PROFESSOR G. T. W. PATRICK.....	174
<i>Societies and Academies:—</i>	
<i>New York Academy of Sciences—Section of Biology:</i> GARY N. CALKINS.	
<i>Section of Geology:</i> PROFESSOR RICHARD E. DODGE.	
<i>Sub-section of Anthropology and Psychology:</i> PROFESSOR CHARLES B. BLISS.	
<i>The Chemical Society of Washington:</i> DR. V. K. CHESNUT.	
<i>The Biological Society of Washington:</i> F. A. LUCAS.	
<i>Boston Society of Natural History:</i> SAMUEL HENSHAW.....	176
<i>New Books.....</i>	180

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THE BIOLOGICAL PROBLEMS OF TO-DAY.*

Paleontological Problems. PROFESSOR HENRY F. OSBORN, Columbia University.

THE chief paleontological problems of the present day are involved in the phylogeny of the Mammalia, for upon this depend both Embryology and Comparative Anatomy, as well as Paleontology. The last decade has been one of a rapid succession of brilliant discoveries in South America and in southern Africa, and of a very great expansion of our knowledge of the North American fauna, together with some single discoveries of great importance, chief among which is the discovery of the foot structure of *Psittacotherium* by Wortman, leading to his exposition of the order Ganodonta as ancestral to the Edentata. Of great interest also is the hypothesis recently advanced by Matthew, that *Mixodectes*, of the Basal Eocene, is the ancestor of the Rodentia, instead of being connected with the Primates, as Cope supposed.

As regards the South American forms they are mainly important as revealing the existence of a new life center upon a continental scale; as tending to demonstrate a continental union between South America and Australia, and as exhibiting Marsupials which are more nearly allied to Placentals than any hitherto known. As Lydekker

*Discussion before the annual meeting of the American Society of Naturalists held at Ithaca, N. Y., December 28, 1897.

and Scott, on paleontological grounds, and Hatcher, on geological grounds, have demonstrated, this fauna is by no means of the great antiquity assigned to it by Ameghino. It is rather modern and specialized than central and ancestral. The sources of this fauna should be sought possibly in an overflow of primitive Marsupials from Australia or elsewhere, and partly in an early emigration of Condylarthra from North America. The latter may have constituted the origin of the Litopterna, but they give us no light upon the Toxodontia. At the same time the general principle of North American origin is strongly reinforced by the demonstrated relationship of the Ganodonta to the Edentata.

As regards the remaining Ungulates of the world, the origin of the Proboscidea and Hyracoidea is still wholly unknown. The Sirenia also remain without known ancestors. The group of *Ancylopoda*, proposed by Cope for *Chalicotherium* and other clawed forms with the bodily proportions of the Sloths, but many essential skeletal structures of the Perissodactyls, loses its distinctness from the Perissodactyl phylum, because of the discovery that *Agrichoerus* besides *Diplobune*, both undoubted Artiodactyls, exhibit a very marked parallel specialization of hoofs into claws. Going further back to the Lower Eocene there still exists a break between the Artiodactyla and Perissodactyla and any of the known forms of Condylarthra, for none of the latter are as yet proved to be directly ancestral to the even or odd-toed Ungulates. The Amblypoda stand apart as a very ancient and distinct phylum, geologically the oldest, and in structure the most archaic of all Ungulates; they should include the *Periptychida* and thus embrace the whole range of amblypods from the small arboreal *Periptychids* to the huge clumsy *Uintatheres*.

The most primitive type of Condylarthra (*Euprotogonia*) and of Amblypod (*Panto-*

lambda), as recently studied by Osborn and Matthew, strongly reinforces the hypothesis first enunciated by Cope, *that the source of the Ungulata is to be found in the Creodonta*. Upon the other side of the great Mammalian tree, the numerous branches of Unguiculates or primitive clawed types also have converged towards a Creodont ancestry, as seen especially in the characters of the Ganodonta, or ancestral edentates, and of the Rodentia, if Matthew's supposition proves to be correct; also of the Tillodontia. Thus all these groups should probably be added to the Carnivora as Creodont derivatives. The Carnivora extend back into Creodont prototypes; but, as in the case of the Artiodactyla and Perissodactyla, the actual points of contact or links between these two divisions are yet to be discovered. So, again, with the Primates. Recent embryological evidence has tended to separate the Lemuroid and Anthropoid phyla. Hubrecht is confirmed by others in placing *Tarsius* near the parting of these phyla (although not in his separation of this genus from the Lemurs), and he makes a very radical break between Lemurs and monkeys upon grounds of placentation. The point of contact of the Primates with the Creodonta is still entirely wanting, but their relations appear to be here rather than with the Insectivora.

In spite, therefore, of the many remaining deficiencies or absence of links in our paleontological evidence, it has none the less come about *that the Creodont type takes the central position which was assigned by Huxley in 1880 to the Insectivora*, for the known Creodonta are more generalized and more central than any other of the known Insectivora, fossil or living, the known Insectivora showing a very considerable specialization, especially in their dental succession, which places them apart as a distinct side phylum. This does not affect the derivation of the Creodonta themselves from stem forms of unspecialized Insectivora existing in

the Jurassic period, the characters of which are very largely seen in the *Insectivora Primitiva*, or placentals of the Stonesfield Slate and Purbeck periods.

The discoveries in South Africa above alluded to take us back to the still older period of the origin of the Mammalia. Two of the types of the Theromorphs of the Permian and Lower Triassic, namely, the *Theriodontia* and *Gomphodontia*, supply many of the characters which we have expected to find in the ancestry of the Mammals. In fact, they embrace the few osteological characters placed in Haeckel's Promammalia, or Huxley's Hypotheria, as well as the more numerous characters which we have subsequently put into the Mammalian archetype. The *Theriodontia* resemble in their dentition and structure the minute *Protodonta* described by Osborn from the Triassic, but differ in the compound character of the jawbones as well as in their surpassing size. In tooth structure they are also prototypes of the *Triconodonta* or Marsupials of the Jurassic period. On the other hand, the herbivorous *Gomphodontia*, including *Tritylodon*, are prototypes of the great phylum of Multituberculata, which in turn, upon extremely slender evidence however, have been associated with the Monotremata.

Thus while the phylogeny of the Mammalia is still in a highly incomplete, speculative and shifting condition, if compared with the evidence we could have mustered ten years ago, it marks a prodigious advance and is full of stimulus for the immediate future of paleontology.

Botany. PROFESSOR WM. TRELEASE, Missouri Botanical Garden, St. Louis, Mo.

THOUGH for a time I found opportunity for work along ecological lines, necessity has compelled me to confine my study, of recent years, so closely to descriptive botany that at first I felt some hesitancy

in accepting the invitation to open this discussion of the biological problems and proposed methods for their solution, in botany. But, on second thought, I decided that I might, without impropriety, do so, since I recalled the statement, heard many years ago on this campus, that the ultimate systematic arrangement of living things will be at once an epitome of all that is known of them and a key to their entire history; and I fully recognize that many of the most serious problems confronting the descriptive naturalist to-day are to receive their solution through increased knowledge of the things studied as living things. In point of fact, the great problem for the botanist and zoologist, the problem underlying and running through all others, is the problem of life.

I have seen so many vital phenomena explained by normal, if complex, physical laws that I may be pardoned, I trust, if at the outset I state that I look at this problem as a physicist and not as a vitalist, feeling that, with each added physical demonstration given, the improbability of an extra-physical answer to each unanswered question becomes in an even greater degree unlikely.

That which the botanist and zoologist are primarily concerned with is protoplasm. In general essentials alike in animal and plant, yet in detail differing in two individuals of the same species, in the twin offspring of the same parent, in different organs of the same organism, and seemingly in the same living cell at different periods, differentiated so that, at least in the vegetable kingdom, the morphological unit, the cell, is yet a complex organism, this substance represents apparently a most complex and ever-changing mixture of most complex and unstable organic compounds.

Though the animal possesses a higher specialization and a greater corresponding differentiation of its cells, and though those

which are exposed, or upon which much devolves, are here bound together by a wonderfully developed class of nerve cells, along which, from the center of sensation, travels an impulse which, through terminal dendrites, may establish and re-establish itself in that wonderful phenomenon which we call memory, and though the metabolic processes connected with the maintenance of animal temperature and with nerve structure and nerve action are more complex and less differentiable than in the vegetable kingdom, so that the plant is frequently turned to for an illustration of simple cell action; the green cell performs that added function of photosynthetically recombining the elements of simple unassimilable compounds into assimilable organic compounds, which by specialized structures are converted into organized substance, which again, by the action of secreted enzymes, is digested and fitted for transportation to points where it may be wanted for use, while these same compounds are still further synthetized by the incorporation of nitrogen, for the most part in relatively simple organic combination, so that it is by no means certain that the simplest field for the study of protoplasmic activity is afforded by the plant. Here, then, in the nutritive changes induced by and occurring in this delicately balanced vehicle of vital manifestations, lies the seat of one great problem: Is life life, or is it an attribute of matter? Does the synthesis of organic matter stop with the formation of the vegetable carbohydrate or the vegetable reserve proteid, or does the one pass into the other, which in its turn grades into the living protoplasm of the cell, the molecules of which, during active life, undergo continuous mutations and shifting combinations from the nutrient to the living and from the living to the excreted form? A part of this question has been answered. What shall be the answer to the other part? and

if physical, what is to be said as to a positive suspension of protoplasmic activity, amounting to functional death, and of a revivification of protoplasm which actually has been dead?

One must concede that in plants, as in animals, death inevitably comes, sooner or later—unless one chooses to juggle with terms in an effort to prove that the unicellular organism, the individual cambium cell and the like are immortal. But in what does it consist?

In medicine a system of pathology has been worked out by which the theory and practice of a generation ago have become the science and art of to-day. For plants a science of pathology just as complicated, just as useful for the preservation of the life of the individual, remains to be worked out. Does disease cause 'loss of vitality,' or is this merely an expression of imperfect nutrition or clogging by waste products? What is anæsthesia? Is it a temporary reduction of vitality in certain cells, or an enveloping of their molecules by the inhibiting agent or its derivatives?

What is reproduction? What is heredity? The vehicle of each, as of every other vital phenomenon, is protoplasm; more, it is known that nuclein is directly concerned in the reproductive processes, and the technique of to-day has enabled it to be shown, for plants as for animals, that certain parts of certain cells unite. The physical or visible basis for a theory of the transmission of characters is more nearly reached to-day than ever before, but is the real essence of the problem any nearer elucidation? Why does the fertilized gamete of the alga produce a seaweed, and of the phanerogam a flowering plant? Why does the meristem of the oak produce oak leaves on all branches?

An analysis and subanalysis, to the last degree, of all of those phenomena which we call vital, and a chain of experiments elim-

inating successively each of the factors which can affect any vital process, can alone give answer. We may not live to see it, but perhaps it is not impossible that, though not a spontaneous generation of organisms, a planned generation of living matter may be effected under the eye of the experimenter.

Of the grosser directly biologic problems facing the botanist, none is more simple in appearance, nor apparently more difficult of solution, than that attending the rise of crude sap from the root to the leaf of one of the higher plants. Purely physical in the wonderful osmotic action of the absorbent cells, and purely physical in the evaporative action of the foliage, the flow of sap has a middle part to which the laws of physics elsewhere have not been fitted; and yet this conduction in the main is carried on through tissues which are dead. Here, too, the isolation, one by one, of all disturbing possibilities offers the only control of experiments from which final conclusions are to be drawn.

The plant has not a nerve system. It is true that its protoplasm communicates from cell to cell through all of the living parts, but no differentiated chain of corpuscles exists for the transmission of sensation, or whatever else you choose to call it, from organ to organ, much less from operative organs to a central control organ; and yet there are plants which are called irritable or sensitive; organs which, if touched, coil about a support—clasp, for digestive purposes, prey; leaves which, for protective purposes, drop into an inconspicuous position, or into a position exposing them less to the heat of the mid-day sun or radiation into the cold of night. These movements are said to be reactions to stimuli, manifestations of protoplasmic irritability; but those who have looked deepest into them find the difficulties of explaining the exact process multiplied the

further they go. Division of the problem, division of labor, experimentation and observation under conditions most favorable for the normal growth of the plant, are the means of reaching a solution.

We owe it chiefly to Darwin that a science of ecology has sprung into life. The German school would call it biology, but it is not precisely what is immediately considered here as biology. It is the interrelations between living things and between them and their surroundings. All that, with loose expression of teleological purpose, would be called 'adaptation' belongs here. Many facts are well known. The theories advanced for their explanation often seem to explain them, but the theories concerning their origin are not always so satisfactory. Who can say that with more knowledge we may not discard even the most fundamental of them? Observation and differential experimentation are here means to the end, no less than elsewhere. How do plants react to their surroundings in nature—under cultivation? How have their species come into existence in their present form? The general fact that they do react, and that they have been evolved from preëxistent types by a process in some degree of the survival of the fittest, is currently believed. The horticulturist to-day produces what he openly calls species in the vegetable kingdom. Are not his methods indicative of the line to pursue in answering the more recondite questions of descent and multiplication?

In conclusion, to come to that in which more nearly I myself am compelled to work, I wish to state that the study of local floras—the study of the flora of one's back yard in a city, of his stone wall, of the roof of his house, if we have an old house, of an old cheese-box—is far from being a mere determination and enumeration of the several species represented. It is becoming a census of the individuals, an

investigation of the communities that they form, and of the interlocking of these into greater, more complex communities; a study of the external configuration of individuals, with reference to their resistance to undue humidity, undue dryness, unusual cold, extreme heat; an anatomical study of their several organs as connected with the same factors; a chemical study of their secretions in the same light; and, finally, a return to that with which I began, a study of their protoplasm in all its phases.

Anatomy: What is the Morphologic Status of the Olfactory Portion of the Brain? PROFESSOR BURT G. WILDER.

IN view of the multitude of problems now confronting anatomists,* it has seemed to me that the present occasion may be best utilized by discussing, in some detail, a single topic which has, nevertheless, intimate relations with several others in anatomy and embryology, human and comparative. Most of the points are indicated upon the wall-maps exhibited.†

Stated more specifically, does *the olfactory*

*In 1894 I stated (Records of the Association of American Anatomists, sixth meeting, p. 32) that, in addition to about fifty special questions respecting each of the fifty particular cerebral fissures, there are at least one hundred general problems connected with them as a group of features of what is commonly mentioned as a single organ.

†These included diagrams of the brains of man, sparrow, turtle, *Necturus*, *Ceratodus*, *Scymnus* (after T. J. Parker), *Chimæra*, *Polyodon*, *Petromyzon* and *Bdellostoma*: a diagram of the mesal aspect of the human thalamus, etc., exhibiting the location of the aulix ('sulcus Monroi') as first described by Reichert, together with the deflection of its cephalic half as proposed by His; and schemas representing (a) the dorsal aspect of the six definitive segments now recognized by me; viz.: Rhinencephal, Prosencephal, Diencephal, Mesencephal, Epencephal, Metencephal; (b) the same as if medisectioned; (c) the several brain flexures, especially the diencephalic; (d) the five different topographic relations to the general axis of the brain (as represented by the olfactory crus) of the presumed psychic expansions.

portion of the brain constitute a definitive segment; or does it, together with the striatum and pallidum, constitute merely the 'dorsal zone' of a segment whose ventral zone is the 'pars optica hypothalami,' i. e., the region about the chiasma?

As a basis for the consideration of this question are offered the following propositions, the validity of which each must determine for himself:

1. We must distinguish between the *potential neuromeres*, the precise number of which may not be determined for decades, and the *definitive segments*, which are convenient and natural divisions, even if not all of equal morphologic value.

2. For the determination of the segmental constitution of the brain more reliance is to be placed upon comparative anatomy and embryology than upon the structure and development of that morphologic monstrosity, the human brain.

3. The recent enactments of the Anatomische Gesellschaft upon this subject (B. N. A., 1895) are based almost exclusively upon the conditions in a single member of the vertebrate community, man; at the best, even if they apply more or less closely to the other mammals, they constitute an example of 'class-legislation.'

4. When a writer employs a term in a sense other than either (a) that which is generally accepted, or (b) that in which it was first introduced, or (c) that in which it is used by other writers whose views he may be discussing, it is incumbent upon him to state explicitly the sense in which he proposes to use it.

The present obstacles to the recognition of a rhinencephalic segment are three, viz.: (1) The common impression as to the insignificance of the olfactory region. (2) The existence, in the higher vertebrates, of the modification designated by me as the diencephalic flexure. (3) The adverse view adopted in the B. N. A., based largely upon the assumption that the region

cephalad of the mesencephal comprises dorsal and ventral zones demarcated by an alleged sulcus connecting the mesocele with the *recessus opticus*.

1. Doubtless all members of this society have discarded the anthropotomic estimate of the olfactory bulbs and their crura as constituting merely a 'first pair of cerebral nerves.' But not all, perhaps, fully realize that, notwithstanding their complete absence in certain adult Cetacea, in most Mammals the olfactory bulbs are quite massive; that in Batrachians, Reptiles and most Selachians they constitute a large proportion of the brain; and that in lampreys and hags they equal in size 'the cerebral hemispheres.'

Had the study of the vertebrate brain begun with *Myxine* or *Bdellostoma* the olfactory bulbs would have been unhesitatingly assigned a rank at least equal to that of either of the three following subdivisions.

Whatever the ontogeny in a given case, it is probable that phylogenetically the smelling portion of the brain preceded the reflective.

"The revolution, so to speak, of the 'hemisphere' about the olfactory axis accords with other considerations which have led Spitzka and the writer independently to consider the prevailing idea that the olfactory lobes are mere appendages of the cerebrum as nearly the reverse of the truth."*

2. The Diencephalic Flexure. With Reptiles, Birds and Mammals, the forms with which most anatomists are more familiar, the first (cephalic or 'anterior') of the series of cavities seems to be the '*ventriculus tertius*'; indeed, in some Birds and Mammals the recess at the root of the optic nerve actually lies farthest cephalad. This condition seems to be associated with the gen-

eral crowding of the cerebrum dorsad and caudad over the other parts of the brain. It is discussed briefly in the *American Association Proceedings*, 1887, pp. 250-251; *American Naturalist*, October, 1, 1887, 914-917; Reference Handbook of the Medical Sciences, VIII., 112, and *Journal of Comparative Neurology*, VI., 128.

The following propositions seem to me warranted by the conditions in Batrachians and 'fishes:'

However numerous or sharp the dorso-ventral flexures of a given brain, for comparison with other brains or with an ideal schema the axis is to be regarded as straight.

Whatever its actual position, the aula or mesal space between the two portas ('foramina of Monro') constitutes the cephalic member of a longitudinal series of cavities.

From the standpoint of comparative neurology the terma ('*lamina terminalis*') is a constituent of the floor of the encephalic cavities; its dorso-ventral position in Reptiles, Birds and Mammals no more converts it into a morphologic end-wall of those cavities than its dorso-caudal inclination in certain forms entitles it to be interpreted as a portion of the roof.

3. In order to be entitled to rank as a definitive segment must a given region exhibit the dorsal and ventral zones of His?

Conceding, for the present, the constancy and significance of these zones in the myel (spinal cord) and in the brain as far as the cephalic orifice of the mesocele ('aqueduct'), are they represented in the region beyond?

In the absence of complete developmental and histologic evidence on that point, my provisional answer in the negative is based upon two very different considerations:

First, the general distinctions between the parts derived from the first encephalic vesicle and the rest of the cerebro-spinal axis. *Secondly*, the unsatisfactory presentation of

* The Dipnoan Brain, *American Naturalist*, June, 1887, p. 546.

the subject by those who attach most importance to it.

In 1859 and 1861 Reichert described and figured (*Der Bau des menschlichen Gehirns*, Plates II., X., XI., p. 65, line 5) a furrow on the mesal aspect of the thalamus, connecting the 'aqueduct' with the *foramen Monroi*. To this he applied the name *sulcus Monroi*, which has been generally employed. In 1884 the mononym *aulix* was proposed by me, and the feature has been shown distinctly in the *New York Medical Journal*, March 21, 1885, p. 327, and 'Reference Handbook,' Vol. VIII., p. 122, and IX., Fig. 418.

In his exposition of the schema adopted by the Anatomische Gesellschaft (B. N. A., pp. 157-159) Professor His insists upon the great morphologic significance of the dorsal and ventral zones, and of the '*sulcus limitans ventriculorum*'* by which they are demarcated. He further declares that the continuation of this sulcus is the *sulcus Monroi*. But his figures represent the sulcus as terminating, not, as with Reichert, at the *foramen Monroi*, but at or near the optic recess, and, without explanation of the radical deflexion, he says, "Die Sulci Monroi laufen jederseits im *Recessus opticus* aus." The confusion caused by this unspecified transfer of a title to a different feature is augmented by the account of the same matter by C. S. Minot in the *Popular Science Monthly*, July, 1893; here the text is explicit as to the importance of the sulcus and its termination at the *foramen Monroi*; but the figure represents the boundary between the zones at a point farther caudad.

In this connection it should be stated that the recent studies of Mrs. S. H. Gage upon embryo cat, turtle, batrachian and bird (*Amer. Nat.*, October, 1896, 837) have revealed sulci having various directions, but not, apparently, demarcating the dorsal and ventral zones.

*For this I have proposed the more definitely correlated name *sulcus interzonalis*.

In view of the present aspect of the case, while I see no impossibility in the representation of the dorsal and ventral zones in the first three segments of the brain, and while such zones might well be demarcated by the furrow originally described by Reichert as '*sulcus Monroi*' (my *aulix*), I hold that the interpretation of the olfactory portion of the brain as merely one part of the dorsal zone of a segment must be supported by something more than the designation of a limiting sulcus which is apparently either non-existent or without interzonal significance.

Psychology. PROFESSOR J. McKEEN CATTELL, Columbia University.

THE speaker said that the knowledge of paleontology, reasonably presupposed by Professor Osborn on the part of all students of natural science, could scarcely be expected in the case of psychology. Neither was it possible to exhibit the whole of psychology on a single blackboard, as Professor Osborn had done for paleontology, or even in a more bewildering series of charts, such as Professor Wilder had found needful for neurology. He could only make some very general, and, he feared, somewhat trivial remarks.

Each science has problems in common with other sciences and problems peculiarly its own. We who are trying, each of us, to advance some little department of science cannot but sometimes stand at gaze before the magnitude of modern science. How can we see the forest for the trees, the library for the books, the world for the facts? Professor Klein has said that mathematics is ten thousand years in advance of the other sciences, but how does he know whether the sciences are an asymptote to his mathematics or whether mathematics are going off on a tangent to the rest of the universe? Professor Klein tells us that to the regular polygon of 65,537

sides Professor Hermes has devoted ten years of his life. It was once a vital question as to how many angels could dance on the point of a needle. Apart from the earmark of material utility, it is not easy to adjust scientific values. We trust that in religion, in art and in science there is, in addition to the transient, the permanent. But it is a problem, and a difficult one, for the soldier in the thick of battle to reflect on international law and constitutional history.

The magnitude and the multiplicity of science suggest a problem that has always been emphasized in this society. Each of us is a teacher :

“ And gladly wolde he lerne and gladly teche.”

But what shall he learn and what teach, what forget and what ignore? Admitting the narrow capacity of a single mind, with what shall it be filled? Each with diverse contents, doubtless, if we are to secure the best results. But what shall be the common property of all—what should we learn and teach in school and college? Certainly none here can ignore the doctrines of evolution ; probably none should neglect the fundamental concepts of physical science ; perhaps we should all know how to use a tool as fine as the calculus. But should a large part of the six or eight years of greatest receptivity be given to Latin and Greek? It is a difficult question. The classics, in our present civilization, are a mark of culture that no one likes to be without. But are they the causes of culture, or only its insignia? Are they to be classed with white linen and polished shoes, possibly even with tight lacing and high heels, or do they give us more life and better?

Turning now to the problems concerning the content of the biological sciences, I venture to maintain that the science of to-day is either quantitative or genetic. Modern physical science is scarcely older than

the doctrine of the conservation of energy—50 years old. Modern biological science may properly date from 1859. The physical sciences then became quantitative, and the biological sciences then became genetic. Earlier, the sciences were largely engaged in giving things names. The zoologist, the botanist, the psychologist, and even the physicist had the naïve faith in names as a method of description of the little girl who remarked that Adam had given a very appropriate name to the hog. We still, I fancy, have a somewhat exaggerated confidence in laws, theories and animistic personifications, as explanations of the development of living things. I believe that the great problem now before biological science is to add to its genetic method the quantitative method of physical science, and thus apply a kind of description, economical and far-reaching beyond all others.

Yet, here another problem arises. When we have our quantitative and causal science, our formula bears about the same relation to the world that it is intended to express as a herbarium does to a primeval forest. Our regard for the body of nature becomes that of the anatomist rather than that of the lover. How can we reduce things to an abstract formula without ignoring their concrete and infinite variety? Fortunately, the subject of this discussion is the biological problems of to-day, not their solution.

As to the problems peculiar to the psychologist, it would be scarcely becoming to bring our family quarrels before the larger public of the biological sciences; besides, they are too numerous to be even mentioned in the latter part of ten minutes. I do not like the term ‘the new psychology.’ I prefer to maintain that psychology is one of the oldest of the sciences. Still, if modern physics is only 50 years old, and modern biology only 40 years old, modern psychology is still younger. I am not as old as I

expect to be some day, but I was, I think, the first professor of psychology as a separate subject, not only in America, but anywhere. When our present psychology is so young, it is natural that there should be difference of opinion, and even confusion, in regard to its scope and methods.

Our great problem, it seems to me, is the one I have already mentioned as common to all the biological sciences—the extension and coördination of the genetic and quantitative methods. And we have really accomplished a great deal. There was no laboratory of psychology in America, and only one in the world, prior to 1883. Now they are everywhere—perhaps forty in American colleges and universities. In nearly all these laboratories experiments are in progress, which are enlarging our knowledge of sensation, of movement, of feeling and of action. Parallel with this development of experimental psychology, bringing our science into fruitful relations with the physical and mathematical sciences, there has been a noteworthy advance in genetic psychology—witness the address of the President of our Association this year—placing mental development in close touch with all the biological sciences. At the same time increased knowledge of the relations of body and mind has made almost a special science of physiological psychology. Degeneration is a phenomenon common to all the biological sciences, but unfortunately one very prominent in the subject-matter of psychology. Here we have a wide field with many points of contact with pathology and medicine. In the interrelations of minds we cross the paths of anthropology, of sociology, of philology and of history. Psychology is concerned with art and with conduct; it is essential to a sane philosophy.

The subject-matter and the problems of psychology are entangled with those of many sciences, but perhaps with none so

closely as with those represented in this discussion. We students of psychology need to know what you are doing, and welcome as a help this affiliation of societies. We hope that you in turn will find that psychology should not be neglected, but that it contributes something to each of the biological sciences and to the advancement of science as a whole.

Physiology. PROFESSOR JACQUES LOEB, University of Chicago.

IF it be true that the fundamental problem of Physics is the constitution of matter, it is equally true that the fundamental problem of Physiology is the constitution of living matter. I think the time has come for Physiology to return to its fundamental problem.

Living matter is a collective term for the qualities common to all living organisms. Comparative Physiology alone enables us to discriminate between the general properties of living matter and the functions of specific organs, such as the blood, the nerves, the sense organs, chlorophyll, etc. Nothing has retarded the progress of Physiology and Pathology more than the neglect of Comparative Physiology. Comparative Physiology shows that secretion is a general function of all living organisms and occurs even where there is no circulation. Hence it was *a priori* false and a waste of time to attempt to explain secretion from the experiments on blood pressure. Oxidation occurs regardless of circulation, and it was *a priori* a waste of time to consider the blood as the seat of oxidation. Comparative Physiology has shown that the reactions of animals to light are identical with the heliotropic phenomena in plants. Hence it is a mistake to ascribe such reactions as the flying of the moth into the flame to specific functions of the brain and the eyes. Sleep is a phenomenon which occurs in insects and plants, and it would

be a waste of time to attempt an explanation of sleep on the basis of phenomena of circulation. The best interests of Physiology and Pathology demand that the systematic development of Comparative Physiology be one of the physiological problems of to-day.

May I be pardoned for calling attention to one special field of Comparative Physiology which I believe to be especially fertile? I refer to the field of Physiological Morphology. I applied this name to the investigation of the connection between the chemical changes and the process of organization in living matter. Two series of facts allow us to connect these two groups of phenomena: (1) The fact that phenomena of fermentation lead to an increase in the number of molecules, and thus bring about an increase of osmotic pressure in the cells. This increase of osmotic pressure is the source of energy for the work of growth. (2) The facts of heteromorphosis, *i. e.*, the possibility of transforming in certain animals one organ into another or substituting one organ for another, through external influences, such as gravitation, contact, light, etc.

The exact and definite determination of life phenomena which are common to plants and animals is only one side of the physiological problem of to-day. The other side is the construction of a mental picture of the constitution of living matter from these general qualities. In this portion of our work we need the aid of physical chemistry and especially of three of its theories: Stereochemistry, van 't Hoff's theory of osmotic pressure and the theory of the dissociation of electrolytes. We know that the peculiar phenomena of oxidation in living matter are determined by fermentative processes, and we venture to say that fermentations form the basis of all life phenomena. It has been demonstrated that

fermentability is a function of the geometrical configuration of the molecule. *Saccharomyces cerevisiæ* is a ferment for such sugars only as have three, or a multiple of three, atoms of carbon in the molecule. Among the Hexaldoses only d-glucose, d-mannose and d-galactose are fermentable, while their stereoisomeres are not fermentable. But the influence of the geometrical configuration goes farther. Voit has suggested, and Cremer has demonstrated, that there is a far-reaching parallelism between the fermentability and assimilation of Carbohydrates. Higher animals as well as yeast cells are able to form glycogen from such carbohydrates as are fermentable by yeast. The further development of these stereochemical relations and their extension to proteids and nucleins is another of the problems of Physiology which will contribute to the main problem, the analysis of the constitution of living matter. I believe that the influence of stereochemistry will be more or less directly felt in many branches of Physiology, in questions of heredity as well as in the theory of space sensations, as E. Mach has already intimated.

Van 't Hoff's theory of osmotic pressure permits an application of the law of conservation of energy to a class of phenomena to which this law was hitherto inapplicable, namely, the phenomena of growth, functional adaptation, secretion, absorption and even pathological processes, such as oedema. The physiologists who thought that the blood pressure determined secretion could not understand why secretion took place under a higher pressure than the blood pressure. Comparative Physiology shows that secretion does not depend upon circulation, and the theory of osmotic pressure indicates that the osmotic pressure in the cells is more than twenty times as high as the blood pressure. The work of secretion is done by osmotic pressure, and not by blood pressure. A

prominent physiological chemist has become a vitalist because he could not explain why the secretions differ from the blood from which he thinks they are formed. He overlooks, among others, the fact that the protoplasm possesses the quality of semipermeability, which means that it allows certain substances to pass through, and others not. In my opinion, the working-out of a theory of semipermeability is one of the main physiological problems of the day.

The theory of the dissociation of Electrolytes is of fundamental importance in the analysis of the constitution of living matter. Pharmacology will feel its influence most directly. Everything seems to indicate that the specific physiological effects of inorganic acids are due to the number of positively charged Hydrogen Ions in the unit of solution, and the specific physiological effects of alkalies to negatively charged Hydroxyl Ions. But the universal bearing of the theory of dissociation upon Physiology will perhaps be best seen in the field of animal electricity. An active element of living matter is negatively electric to its surrounding parts. We may assume that an acid is formed in the active part, and that the passive parts are neutral. The positive Hydrogen Ions of the acid have a much greater velocity of migration than the Anions. Hence the former will diffuse more rapidly into the passive tissue than the Anions, and the active tissue will remain negatively charged.

At no time since the period immediately following the discovery of the law of conservation of energy has the outlook for the progress of Physiology appeared brighter than at present. But in order to reap the full benefit of our opportunities we must bear in mind that the fundamental problem of Physiology is the determination of the constitution of living matter, and that in order to accomplish our task we must make

adequate use of Comparative Physiology as well as Physical Chemistry. Pathology, in particular, will be benefited by such a departure.

Developmental Mechanics. PROFESSOR T. H. MORGAN, Bryn Mawr College.

In the last few years a new movement has started in embryology known as *Entwicklungsmechanik*, developmental mechanics, or rather the mechanics of development. In the few minutes at my disposal I shall try to show :

I. How the term *Entwicklungsmechanik* arose and how it has been defined.

II. I shall try to give an idea of the kind of work that has been done.

Roux, in 1885, first used the word developmental mechanics and defined it as the study of the causal morphology of the organism. It is of importance to note that Roux uses the word mechanics not only in its physical sense, but in its wider philosophical meaning. Therefore, in the definition of developmental mechanics as the study of the causal morphology of the organism, Roux means simply that the changes in form through which the embryo passes are the result of a series of causes, and these causes are what the new study proposes to investigate.

It may seem pretentious to state that this is a new study, for every embryologist must believe that the ultimate goal of his work is to determine, as far as possible, the causes of development. But let us look a little more closely into Roux's position.

Perhaps the problem may appear clearer if we consider it in the form of a concrete example. In what way, for instance, would the study of the mechanics of development differ from ordinary descriptive embryology?

We see the egg segment and then form a blastula, gastrula and larva. Descriptive embryology gives a series of pictures of these

different stages. The more complete the series the fuller will be our knowledge.

Now Hertwig maintains that this knowledge of the successive stages of development is itself causal knowledge beyond which we can not hope to go. He holds that the egg is the *cause* of the blastula and the blastula the cause of the gastrula, etc. Hertwig pretends to be completely satisfied with knowledge of this sort.

Comparison is not perhaps always just, yet Hertwig's position is the same, I think, as though a physicist were to say that if we knew the path of the moon around the earth we should know everything that we could hope to know, or if the astronomer claimed that the position of the moon at one moment is the *cause* of its next position.

Roux, on the other hand, maintains that in order to understand the successive stages of development we must know how the one transforms itself into the other, how the blastula invaginates to form the gastrula, how the medullary plate of the vertebrate embryo rolls in to form a tube. The movements, then, of the parts of the embryo are to be studied. But even a knowledge of the movements of cells and groups of cells would not be causal knowledge, although it might, perhaps, be called the mechanics of the embryo. What makes the endoderm turn in? What induces the medullary plate to roll up into a tube? What, in brief, are the forces at work?

A few illustrations of the kind of work that embryology has already accomplished may bring before us more clearly the problems of to-day.

Pflüger's experiments on the effect of gravity on the segmentation of the egg naturally suggest themselves first. When the egg of the frog is inverted, with its dark hemisphere turned down, the cleavage planes appear, not in their normal position, but in respect to the direction of the force of gravity. At first Pflüger seemed to think that

there is some causal relation between the force of gravity and the forces that direct the cleavage of the egg.

Roux showed, however, that a centrifugal force could replace the force of gravity, and, moreover, that if the experiment were so arranged that the centrifugal force just overcame the force of gravity then the egg segmented normally in whatever position it was placed.

Finally Born showed that a rotation of the contents of the inverted egg occurred so that the lighter parts rose to the highest points. It is obvious, therefore, that gravity only indirectly affects the egg by bringing about a rearrangement of its contents.

This series of experiments is instructive, I think, in that it illustrates how one experiment leads to another, and how our knowledge of the forces at work is advanced with each well-planned experiment. We do not know, to be sure, why the egg segments, but we have found out something definite about the action of gravity on the egg.

Let us now consider another series of experiments: Roux found that by preventing the development of one of the first two blastomeres of the frog's egg the other uninjured blastomere developed into a half-embryo. Naturally enough, he drew the conclusions that the first two cells are self-differentiating, and that the development is, at least in part, a mosaic work. The conclusion was, I believe, not justifiable at the time, because in the experiment the injured half of the egg remained in contact with the developing half.

Later experiments on other forms—the Sea-urchin's egg, for example, where the blastomeres can be completely separated—gave other results. A whole embryo developed from the isolated parts. Roux's conclusions were said to be overthrown. Then came an unexpected result. The blastomeres of the *ctenophor* may be com-

pletely separated, as perfectly as those of the echinoderm, but in the ctenophor the isolated blastomere develops into a half-embryo. Evidently, then, any new theory of development must explain how in one case it is possible for an isolated blastomere to develop into a half-embryo, and how in another case into a whole embryo. Perhaps the explanation is not far to seek, for it has been found that in one and the same egg the blastomere may under certain conditions give rise to a half-embryo, and under other conditions to a whole embryo of half size.

I might, had I time, cite many other experiments: those, for instance, in which a part of the unsegmented egg has been removed; Boveri's experiment in which a non-nucleated piece of an egg is entered by a single spermatozoon and an embryo forms; the experiments and observations of the direction of the nuclear spindle in the dividing cell; the experiments on the effects of different salts on development; the effects of light, heat and electricity on the egg or embryo, etc.

These experimental studies will serve as examples of the kind of work of the new embryology.

The two instances that I have already given—the effect of gravity of the egg, and the behavior of isolated blastomeres—teach us that the greatest precaution must be used before we can know whether a suggested mechanical explanation is really a true explanation. There is, I think, but one way in which we may hope to find out what forces or energies are at work during development, and whether these forces are the same forces known to the chemist and physicist. Only by means of well-planned experiments can we expect by isolation and recombination to discover the forces at work. Here, it seems to me, we find at least the real meaning and strength of developmental mechanics.

I admit freely that developmental mechanics is not a fortunate expression, but, nevertheless, Roux and his school have from the start encouraged experimental methods.

Perhaps it would be more appropriate to call the new work 'experimental physiology' of the embryo, using physiology in a wider sense than that usually given to it. For myself, I think our aim is reached if we use the term experimental embryology.

The history of science teaches us that by means of experiment chemistry and physics have made enormous progress; by means of experiment animal and plant physiology have become more exact, more profound studies than animal and plant morphology, and the department of bacteriology shows how rapidly and surely a study may advance by this method. Therefore, by means of experiment the student of the new embryology hopes to place the study of embryology on a more scientific basis.

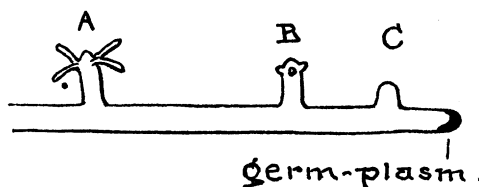
Morphogenesis. DR. CHAS. B. DAVENPORT,
Harvard University.

MORPHOGENESIS may be defined as the study which attempts to explain the development of the form of the individual (ontogenesis) and of the race (phylogenesis).

Morphogenesis is a subdivision of general physiology, inasmuch as it deals with activities—processes, and, indeed, the largest, most complex biological processes, those by which the course of individual development is controlled and the direction of evolution is determined. Morphogenesis includes developmental mechanics in so far as that study attempts to explain the ontogenetic processes.

The scope of morphogenesis, embracing, as it does both ontogenetic and phylogenetic processes, is a broad one. Too broad, some may say who believe that there is no close relation between phylogeny and ontogeny; that ontogeny goes its way and phylogeny

goes its way and neither takes account of the other. Others do not share this view. They look upon the soma and the germ as very intimately bound together—associated in much the same way as the stolon and the hydranths of a hydroid are.



The germ plasm at the tip of the stolon gives rise at intervals to hydranths very much as the germ plasm of other animals gave rise at intervals to somas. In both cases the germ plasm is modifiable to a limited extent by such modifications of the soma as result from starvation, reduction in general vigor, or the secretion of specific substances affecting the germ plasm. In both cases developing soma and germ may be simultaneously modified by external agents, so that while the developing generation C is being changed, future generations, D, E, etc., are being potentially changed in the same fashion because in the germ plasm. For example, although I do not know that the experiment has been tried, a dense solution might produce a spindling soma C and a spindling stolon, so that even if the solution were diluted again a spindling soma D would rise. By other agencies we may modify the protoplasm at the tip of the stolon so that it will thenceforth tend to produce modified hydranths. Just as the formation of the stolon and hydranth are parts of one developmental process, so are phylogenesis and ontogenesis parts of one process. Every ontogenesis is dependent upon a preceding phylogenesis and every phylogenesis is dependent upon a preceding ontogenesis.

I have said that morphogenesis seeks to explain the development of the individual

and the race. When have we explained development? We have explained any effect when we know its immediate causes—that is to say, the essential conditions under which the effect occurs. We seek, then, to know the essential conditions under which phylogenesis and ontogenesis occur.

What general methods must we employ to learn these conditions of development? There are two principal methods: one is the method of observation of the differences in development under known dissimilar conditions; the second method, more applicable and more certain, is the method of experiment.

I may illustrate the way in which simple comparative observation and observation with experiment throw light upon the processes of development. The simple observation that in the tunicate *Doliolum* the sexual buds, detaching themselves from the ventral stolon, crawl over the surface of the animal to the dorsal stolon to arrange themselves there in regular order might have taught us that one of the conditions directing individual development is response of the different parts of the developing individual to stimuli coming from other parts of the organism. On the other hand, the experiments of Driesch upon the gastrula of sea-urchins enforced the fact vividly, for he found that even after the mesenchyme cells had been hopelessly mixed up by shaking the gastrula they still migrated toward their destined place. So, too, the observation of the decline of the descendants of famous men might have led us to the law of regression toward mediocrity as a condition of phylogenesis just as Galton's experiments with sweet peas did. In the foregoing cases there is, however, a precision and decisiveness about the experimental method which marks it as one to be preferred where applicable. In addition to experiment, an allied method applicable especially to the study of variation is that

of statistics. As by experiment we make all causes similar except one and note the result, so in statistics we select results having at least one common cause and throw all together, believing that, from the doctrine of chances, all other causes will offset and annul each other. Thus we find, by comparing the mean in the selected group with the mean of the whole population, the effect of the particular cause used as a basis of selection.

So much for definitions and general methods. But I have been asked to suggest particular problems in morphogenesis and the methods of their solution. Of ontogenetic problems we have the question in how far is the development of the individual to be explained as a series of responses to the action of stimuli; not merely of stimuli external to the organism, but of part acting on part 'as in the marvelous automaton'—to use Aristotle's phrase. We get indirect evidence upon this matter in studying the capacity and laws of response in unicellular organisms; we get direct evidence by applying particular agents, such as light, heat and chemical substances, and noting their effect on development. Again, we have the question in how far the development of the individual is determined by wholly internal factors. To get an answer to this question one must mutilate the form and study the laws and limits of its restoration—regeneration, reparation, healing, development despite untoward conditions (as in dermoid cysts), and self-regulation (or accommodation) in disturbed ontogeny. In how far is the regeneration of organism comparable with that of crystals?

Next we come to a number of problems connected with both ontogeny and phylogeny. Such are the problems of adaptation. There is adaptiveness in those responses to stimuli that are met with in development—in tactisms, tropisms and differentiation.

There is adaptiveness also in regeneration and self-regulation of the organism. These ontogenetic adaptations are often curiously dependent on the past history and habits of the species. Thus, *Amœba* dwells in dim light and is negatively phototactic, ferns of plants which live in the dark turn from the sun, parts of an animal most apt to be lost are frequently those most capable of regeneration. Is it due to selection or is it an inherent quality of all protoplasms that they should respond thus advantageously? Or is this whole phenomenon of adaptation merely an *ignis fatuus*—this apparent shaping of means to ends only a necessary, mechanical relation? These questions can be answered by paying attention to cases of unadaptive response and unadaptive regeneration and regulation.

Finally, the strictly phylogenetic problems deserve far more attention than has yet been given them. Such are the questions concerning individual variation. It is well known that in some cases the measurements of an organ in the different individuals of a species group themselves about a mean value in accordance with the normal probability-of-error curve. In the case of species undergoing change, however, the curve is often very unsymmetrical or perhaps has several maxima. What is the precise meaning, in any case, of these abnormal curves? Again, how does the mode (or the most common measurement) vary with the habitat or geographical position of the varieties of the species? What is the significance of those large variations which we call sports and how do they differ in origin from individual variations? What sorts of variations in the body are correlated? What is the morphogenetic kinship of the various organs of the body? Then there are the questions dealing with inheritance: The laws of normal inheritance—Do the progeny of a particular cross inherit, on the average, equally from the

two parents in all cases—or is there such a thing as sexual or racial prepotency? Do sports show a prepotency in breeding? What are the limits of inheritance—to what extent and to what degree are modifications of the soma transmissible? What are the laws and limits of crossing—the capacity of hybridization; the abnormal distribution—the patchwork intermingling—of parental characters in the body of the adult hybrid? Next there are the questions, allied to those of crossing, respecting the reciprocal effect between scion and stock in grafting. In how far is there such an effect and what is its cause? How about the phenomena of telegency in animals and of xeny in plants? Finally, there are the momentous questions concerning the relative importance of selection, of sporting with segregation of the aberrant individuals, of crossing and hybridization, and of self-adaptation in the origin of species.

Now, these problems are comparatively untouched. Yet they are recognized as immensely important. The reason why they have not been worked upon is largely because they don't lend themselves to investigation in the laboratory. For the successful study of these problems one needs, indeed, not an ordinary laboratory, but a farm or an extensive zoological reserve with hothouses, breeding ponds, insectaries and vivaria of various sorts. With such means at his disposal a naturalist might hope, during a long series of years, to answer many of these fundamental phylogenetic questions.

CURRENT PROBLEMS IN PLANT MORPHOLOGY.

RELATIONSHIP BETWEEN PTERIDOPHYTES AND GYMNOSPERMS.

THE year 1897 will always remain a memorable one in the annals of plant morphology on account of the illuminating dis-

coveries made by Ikeno* and Hirase† of spermatozoids in *Cycas* and *Gingko*, by Webber‡ of spermatozoids in *Zamia*, by Belajeff§|| and Webber¶|| Sm ** of important new facts in spermatogenesis, and by Bower†† of new evidence bearing upon the homologies of spore-producing members.

These investigations, with others somewhat less notable, have already resulted in some important modifications of taxonomic sequence. Engler‡‡ divides the subdivision *Gymnospermæ* into two series—(a) those with functional spermatozoids, including here the *Cycadaceæ*, *Ginkgoaceæ* and fossil *Bennettitaceæ* and *Cordaitaceæ*, each order having also the rank of a class, and (b) those with reduced spermatozoids (*Spermakerne*), including the classes *Coniferæ* and *Gnetales*. Thus the aberrant genus *Gingko* has been removed from the order *Taxaceæ* of the *Coniferæ* and made the type of a new order, which constitutes a

* Ikeno, S. Vorläufige Mittheilungen über die Spermatozoiden bei *Cycas revoluta*. *Bot. Centralb.* 69:1-3. Ja. 1897.

† Hirase, S. Untersuchungen über das Verhalten des Pollens von *Gingko biloba*. *Bot. Centralb.* 69:33-35. Ja. 1897.

‡ Webber, H. J. Peculiar Structures occurring in the Pollen tube of *Zamia*. *Bot. Gaz.* 23: 458. note. Je. 1897.

§ Belajeff, W. Ueber den Nebenkern in Spermato-genen Zellen und die Spermatogenese bei den Farnkräutern. *Ber. Deutsch. Bot. Gesellsch.* 15: 337-339. 27 J1. 1897.

|| Belajeff, W. Ueber die Spermatogenese bei den Schachtelhalmen. *Ber. Deutsch. Bot. Gesellsch.* 15:339-342. 27 J1. 1897.

¶ Webber, H. J. The Development of the Antherozoids of *Zamia*. *Bot. Gaz.* 24:16-22. 31 J1. 1897.

** Webber, H. J. Notes on the Fecundation of *Zamia* and the Pollen tube apparatus of *Gingko*. *Bot. Gaz.* 24:225-235. 30. O. 1897. (See also Webber ‡.)

†† Bower, F. O. Studies in the Morphology of Spore-producing members. The *Marattiaceæ*. Lond. 1897.

‡‡ Engler, A. Nachtrag zu Teil II.-IV. *Pflanzenham.* 341. 1897.